more detail, but I feel that a reassurance to patients who have had this most successful procedure carried out is necessary at this stage.

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1 McKenzie, A W, Aitken, C V E, and Ridsdill-Smith, R, *British Medical Journal*, 1967, **4**, 36.

SIR,—We read with interest the papers of Mr M K D Benson and others and Dr M W Elves and others (15 November, pp 374 and 376 respectively). We can confirm that since our paper on the subject of cobalt toxicity in relation to McKee hip arthroplasty[1] was published we also have found that approximately one-third of patients with a metal to metal (cobalt-chrome-molybdenum alloy) prostheses in situ will give a positive reaction to one of the metals of skin testing.

Of 20 patients so far reviewed, four are cobalt-positive, two nickel-positive, and one cobalt- and nickel-positive. The significance of a positive patch test remains unproved, however, and neither of the articles you have published have provided sufficient histological, immunological, or radiological evidence to permit any firm conclusion. Of the patients reviewed by us, there have been several with a positive skin patch test and yet perfect hip function. This is also the experience in at least one other centre.[2]

There are at least two points arising which merit further discussion. Firstly, metallic cobalt and cobalt salts are not only irritants but are directly toxic and this in itself may be significant in the production of any necrotic reaction around the joint. Also, polymethacrylate cement is not beyond suspicion as a possible cause of necrotic reaction. It is important that conclusions such as that of Mr Benson and his colleagues that "it therefore seems advisable to use Charnley or other forms of metal-to-plastic prostheses in preference to metal-to-metal ones" should not be made on the basis of a statistical relationship between skin sensitivity and loosening of the prostheses, particularly when only two cases of loosening are mentioned, neither of which is described nor is the orthopaedic evidence for loosening presented. We are more in agreement with Dr Elves and his colleagues and the guarded conclusion in their paper.

Secondly, we feel it is of great importance that authors on this subject state clearly which metal is used in the metal-to-plastic joints. Thus the femoral component of the Charnley prostheses is produced in stainless steel (for example, Thackray) and cobalt chrome alloy (for example, Down Bros). It may be that it is the use of stainless steel articulating with high-density polyethylene that leads to a low rate of increased metal sensitivity rather than the fact that it is a metal-to-plastic joint. We would therefore ask all orthopaedic surgeons who use the metal-to-plastic prostheses to document clearly in their case notes the particular material used in the femoral component as this may be of great significance at a later date. In this centre the information is being documented in the theatre register.

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1 Jones, D A, *et al*, *Journal of Bone and Joint Surgery*, 1975, **57B**, 294.
2 Ring, P A, personal communication, 1974.

Use of clonazepam in epilepsy

SIR,—In his article on epilepsy (1 November, p 270) Dr F B Gibberd did not mention the use of the newer benzodiazepine anticonvulsant clonazepam, which has been shown to be effective both in the control of seizures in childhood[1] and in intractable epilepsy.[2] This drug may also be useful in controlling status epilepticus without resort to the use of anaesthesia, muscle relaxants, and positive pressure ventilation, as the following brief clinical summary illustrates.

A 6-year-old boy with epilepsy and mental handicap following encephalitis at 4 months of age was admitted in status epilepticus in February 1975. Seizures were well controlled with intravenous diazepam and intramuscular paraldehyde for four days. Thereafter they recurred despite the continuation of this therapy, and treatment with intravenous clonazepam was started. An initial dose of 3 mg in a 500-ml infusion of 5% dextrose was ineffective, but by gradually increasing the dose over the next 48 hours the seizures were controlled. A total of 43 mg of clonazepam was infused. Despite this high dose there was no evidence of respiratory depression, hypotonia, or excessive bronchial secretion. Subsequently the dose was reduced and good control was maintained with 3 mg daily by mouth in combination with phenobarbitone.

Although further experience with clonazepam is required, it may have a useful place in the control of status epilepticus, as previous reports have suggested.[3 4]

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1 Martin, D, and Hirt, H R, *Neuropädiatrie*, 1973, **4**, 245.
2 Hooshmand, H, *Archives of Neurology*, 1972, **27**, 205.
3 Gastaut, H, *et al*, *Epilepsia*, 1971, **12**, 197.
4 Ketz, E, *et al*, *Acta Neurologica Scandinavica*, 1973, suppl 53, p 47.

"Syrup of ipecacuanha"

SIR,—Your leading article on childhood poisoning (29 November, p 483) deserves wide support for its recommendation that doctors, nurses, and ambulancemen should keep syrup of ipecacuanha at hand. It stores well and works well, but care must be taken to obtain the correct syrup. The American original is ipecac syrup *USP*. This can readily be made to order, but some pharmacists are not familiar with it. To ask for an unfamiliar preparation is to risk being supplied with ipecacuanha liquid extract *BP*, 15 ml of which would be a massive overdose. Deaths have been caused by this mistake.[1 2] I have heard of a recent occasion when the liquid extract was supplied but was not administered.

The British syrup carries the cumbersome title "ipecacuanha emetic draught, paediatric *BPC*." This is clearly unsuitable for everyday verbal use and a simple name should be made official.

M S Fraser

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1 Smith, R P, and Smith, D M, *New England Journal of Medicine*, 1961, **265**, 523.
2 Bates, T, and Grunwaldt, E, *American Journal of Diseases of Children*, 1962, **103**, 169.

*** Dr Fraser sent a copy of this letter to the secretary of the *BPC* Revision Committee, whose reply is printed below.—ED, *BMJ*.

SIR,—Ipecac syrup *USP*, on which the original work was based, is not readily available in this country, but ipecacuanha emetic draught, paediatric *BPC* is almost identical and is easily prepared from readily available ingredients using the formula given in the *BPC*. At the time when this preparation was introduced the Codex Revision Committee was aware that misunderstandings had arisen over the meaning of "ipecacuanha syrup" and considered that the use of the term "syrup" for such medicines should be discouraged, as medicated syrups are commonly syrupy stock solutions of drugs for use in extemporaneous preparations. It decided that the title should provide an exact description of the product, the use of which would avoid any possibility of misinterpretation.

At the time of compilation of the *BPC 1973* it was considered that the draught should be given only under medical supervision and not included in first-aid kits, and a statement to that effect appears on p 662. The addition of the preparation to first-aid kits would reinforce the need for a precise title to appear on every bottle so as to avoid any misunderstanding as to the purpose of the contents. However, I agree that there are many occasions when a short title would be useful and I shall see that this is considered when the monograph is revised.

G R Brown
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Codex Revision Committee

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Streptococcus mutans and dental caries

SIR,—I read your leading article "Immunisation against dental caries" (22 November, p 424) with personal interest, but I would like to take you up on one small point.

In mentioning the fact that evidence is accumulating that *Streptococcus mutans* may be associated with dental caries in man you give three references. You then go on to say, "Such a role had been suggested originally in 1924." It was my late father who first described this organism in 1924 when working in the laboratories of Sir Almroth Wright at St Mary's Hospital with a grant from the Medical Research Council.[1] He was attempting to determine the role played by *Bacillus acidophilus* (one of the lactobacilli thought to be implicated) in the aetiology of dental caries. It was found that *B acidophilus* could be isolated only from teeth in which cavities were already formed or in which the foci of caries were shallow and contamination from the surface could not be excluded. During the experiments he grew from culture an organism not previously described to which he gave the name *Str mutans*. This, he found, grew best in a medium with a reaction approximating to that of saliva; it was found in the earliest stages of decay and could be isolated in pure culture from carious dentine which is more or less effectually protected from secondary infection or surface contamination.

Since it is hardly my father's fault that

this work was virtually ignored for 40 years, I think at least he deserves the courtesy of a reference.

T K CLARKE

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1 Clarke, J K, *British Journal of Experimental Pathology*, 1924, **5**, 141.

Oculocutaneous reactions to beta-blocking drugs

SIR,—The "alternative explanation" produced by Drs P M Gaylarde and I Sarkany (16 August, p 435), while plausible, does not in fact stand up to critical evaluation. In the first place there have been very few reports of the oculocutaneous reaction in patients on any of the beta-blocking agents apart from practolol, and most of those in whom oxprenolol or propanolol has been incriminated had also received practolol in the past. Ten of the 21 patients reported by Felix *et al*[1] subsequently received oxprenolol and four were given propanolol and suffered no adverse effect, although 12 of the group were challenged with practolol and developed reactions.

The clinical features of the oculocutaneous syndrome, the development of a positive antinuclear factor test, and the irregularity of the occurrence of this condition are very much in favour of an immunological rather than a pharmacological mechanism being involved. Not that they are mutually exclusive. However, the evidence adduced in favour of a possible pharmacological mechanism is faulty.

It is unfortunate that the term "psoriasiform" has stuck to the cutaneous component of this condition. It is also lichenoid, erythematous, and eczematous at times. Histologically, the lesion shows evidence of intraepidermal cell death and there is not the typical hypertrophy of psoriasis. One certainly cannot assume that increased epidermopoiesis is playing an important role in the pathogenesis of the epidermal disorder. While the importance of the cyclic nucleotides as regulators of cell division in some systems cannot be denied, their role in the control of epidermopoiesis has not as yet been firmly established. Our own experience with phosphodiesterase inhibitors and that of others with other compounds active in the adenyl cyclase—cyclic nucleotide system as depressants of epidermopoiesis and as clinically effective agents in psoriasis has been disappointing.

There would appear to be good evidence for the involvement of immune mechanisms in the pathogenesis of the oculocutaneous syndrome and very little in favour of a pharmacological mechanism.

RONALD MARKS

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1 Felix, R H, Ive, F A, and Dahl, M G C, *British Medical Journal*, 1974, **4**, 321.

SIR,—We read with great interest the case report by Mr R B Cubey and Dr S H Taylor (8 November, p 327) of an ocular reaction to propranolol and its subsequent resolution on continued treatment with a different beta-blocker. We have held for four years a special clinic for patients with coronary arterial disease and have seen no oculocutaneous reactions to propranolol, nor indeed to timolol maleate (Blocadren). Routine ophthalmic examinations have been carried out in the clinic for the four years, and in the event of any suspicion of an adverse reaction the patient is then screened by the ophthalmologists in the normal manner.[1]

There was no reference in the case report to the autoimmune status of the patient at the time that the ocular reaction was found or subsequently. We believe that determination of antinuclear factor may detect early adverse reactions, and other workers have found the antinuclear factor (ANF) titre to be elevated.[1 2] There may well be a place for screening patients before starting treatment with the newer beta-blockers and indeed, since Mr Cubey and Dr Taylor's report, also those on long-term propranolol therapy.

We have found no significant ANF titre in 12 anginal patients who have been on timolol continuously for four years. Similarly, they have had no oculocutaneous reactions.

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1 Wright, P, *British Medical Journal*, 1975, **1**, 595.
2 Raftery, E B, and Denman, A M, *British Medical Journal*, 1973, **2**, 452.

Enteric-coated aspirin overdose and gastric perforation

SIR,—Dr R J Farrand (15 November, p 409) suggests that I am simplistic and jump to immediate conclusions. To the contrary, I still do not have sufficient information even to understand the "facts" in their apparently clearcut case. May I put some unanswered doubts on paper?

(1) The patient clearly ingested 67 Safapryn tablets. To assess whether an 83-year-old patient with a 16-g overdose of paracetamol would behave like some of the rather younger patients in the paper he quotes one needs to know that there was no biochemical evidence of previous hepatic or renal disease or that these organs were histologically normal at post-mortem and showed no evidence of paracetamol toxicity.

(2) Besides the six different drugs mentioned in the paper, the post-mortem report specifies an additional four: propoxyphene (in Distalgesic), digoxin, frusemide, and effervescent potassium as well as the bile-stained silver threepenny bit found in the stomach as mentioned by Dr F J A Bateman (15 November, p 409). If the patient, while in hospital, was able to accumulate 67 Safapryn tablets, recognised by their residues, how did Dr Farrand and his colleagues exclude the others, all soluble in the stomach? He gives evidence only for phenylbutazone in his letter.

(3) From Dr Bateman's letter I realise that the point of the report was that sufficient aspirin may be leached out to cause local irritation despite an apparently intact coat. Did Dr Farrand check the aspirin content of the original capsules or look for salicylate after his elegant experiment with Safapryn tablets, simulated gastric juice, and a 1913 silver threepenny bit? Such a simple test would have proved or disproved his theory.

(4) Dr Farrand excluded phenylbutazone as a cause of gastric ulceration as there was no history of phenylbutazone intolerance. Those who use aspirin, phenylbutazone, indomethacin, etc, regularly know only too well that peptic ulcers or gastric perforation may occur without any previous intolerance.

Dr Farrand asks "How much room for speculation is there in a 'short report'?" That of course depends on the editor, but the authors must ensure that sufficient facts are included to allow readers to understand the authors' conclusions.

Even with Dr Farrand's amplification, I feel the original report belongs to the apocrypha of aspirin toxicity, and in my book it is firmly inscribed as the tale of the bile-stained silver threepenny bit.

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Nutrition rehabilitation units

SIR,—Nutrition rehabilitation (NR) is a practical form of health education aimed at the mothers of malnourished children (1 November, p 246). Wherever such children are being treated, particularly in the paediatric wards of teaching, city, and district hospitals, the management is incomplete without a practical educational component to combat the high recurrence rate.[1] An associated NR unit is not a luxury for it can greatly increase the effectiveness of these children's wards.

Several further points should be noted. An important additional benefit of NR is the practical nutrition training for all members of the medical, nursing, and supporting staff. Too often their nutrition instruction has been only in theoretical terms of the major dietary components, megajoules (Calories), international units of vitamins, etc. Many staff members have never seen such figures in actual volumes of local foods. If they have to spend a period of in-service training in NR units they are better equipped not only to manage malnutrition but also to advise mothers about feeding children in a realistic way.

Both the day-care and residential forms of NR have limitations because no country with large numbers of malnourished children can provide enough NR centres to care for all who need help. In this context domiciliary NR as developed by Professor P M Shah is worthy of consideration.[2]

Evaluation of the effect of NR is complicated because of multiple factors which influence nutrition in an individual family or community. Currently an attempt is being made to test a questionnaire that will help the staff of NR units to assess the effectiveness of their own work.[3] The results of follow-up after rehabilitation are much more encouraging than those after standard hospital treatment[1 4] (see table).

Two groups of workers have specifically studied the evaluation of NR.[5 6] These were both retrospective studies, but Beaudry-Darisme and Latham examined experimental (nutrition-rehabilitated) and control groups of children in Haiti and Guatemala. At follow-up in Haiti the weight-age of the experimental group was significantly higher than that of the control group. This was not the case in Guatemala and this was partly attributed to undue emphasis on the feeding and not enough on maternal education. However, the mortality in the experimental group was much lower than in the control group in both countries—4% and 14% respectively in Haiti and 1% and 9% respectively in Guatemala.

A practical educational component is